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THE STATUS OF THE NONLINEAR THEORY OF DRIFT AND TRAPPED PARTICL--ETC(U)
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The Status of the Nonlinear Theory of Drift and Trapped Particle Instabilities

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The following is an edited text of a letter mailed from the author to Walter Sadowski of ERDA concerning the status of the nonlinear theory drift and trapped particle instabilities.
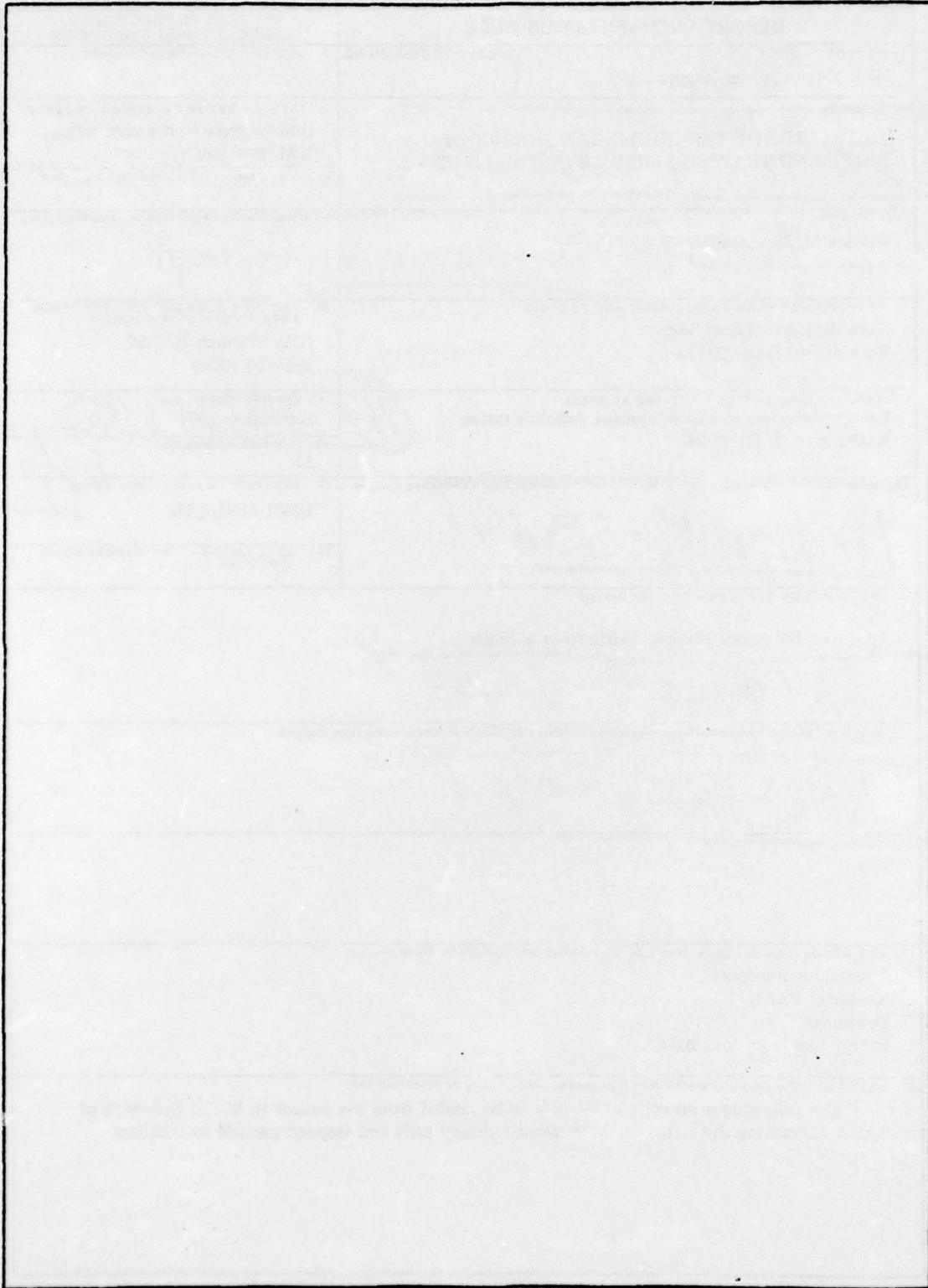
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THE STATUS OF THE NONLINEAR THEORY OF DRIFT AND TRAPPED PARTICLE INSTABILITIES

"But he's not wearing anything!" said the little boy
from THE EMPERORS NEW CLOTHES

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THE STATUS OF THE NONLINEAR THEORY OF DRIFT
AND TRAPPED PARTICLE INSTABILITIES

This note is a short appraisal of the status of nonlinear theories for drift waves and trapped particle instabilities as seen by the author. Such instabilities are thought to play a crucial role in the energy confinement capability of tokamaks.[1] First I will discuss some of the phenomenological[2] and strong turbulence theories[3-5] and then go on to the more quantitative weak turbulence theories.[6-8] *are discussed followed by a discussion of*

Of the phenomenological theories, the most widely used general concept[2] is that saturation occurs when the drift wave or trapped particle instability induced radial density gradient, $\sim k_r \, \delta n$ is comparable to the main density gradient n_o/L_n. When this is so, according to conventional wisdom, the wave no longer sees the gradient which drives it, and therefore saturates. This argument may not make any sense at all. Even if the argument is potentially sound, it cannot really be applied quantitatively to a tokamak plasma.

On the first point, it is important to realize that the wave induced density gradient oscillates at the wave frequency. Thus if $\nabla_r \delta n \sim n_o/L_n$ at one point in the oscillation cycle, so that there is no density gradient, then half an oscillation later, $\nabla_r \delta n \sim -n_o/Ln$, so that the density gradient is twice as big and the plasma is twice as unstable. To this author, the most reasonable thing seems to be that the wave will somehow average over these fluctuations in gradient scale length and pick out some average value (i.e. nearly the original density gradient). In any case, while linear theory <u>may</u> break down when $\nabla_r \delta n \sim n_o/L_n$, this does not mean saturation will occur at the breakdown point. It is worth pointing out that such an argument fails for a current driven ion acoustic instability. If the current velocity is V_D, one might argue that when the oscillating velocity of an electron at V_D, is of order V_D, these electrons do not see the current, so the instability should saturate. This argument would give $e\varphi/T_e \sim (V_D/V_e)^2$. In fact, theory and simulations of ion acoustic instabilities consistently indicate saturation by ion trapping at much larger values of $e\varphi/T_e$.

Even if one buys the concept of wave induced gradients cancelling ambinet gradients, it is not clear how one applies the theory. For instance trapped electron instabilities are driven principally by electron temperature gradients.

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Should not the stabilization condition then be $\nabla_r \delta T_e \sim T_{eo}/L_T$? If so, much larger fluctuation amplitudes are indicated because generally $L_T < L_n$, and also $\delta T_e/T_e \ll \delta n/n_o$ as a result of the good thermal conduction along field lines. Another important point is that ∇_r of the fluctuation is not known since the radial structure of the mode is still not well understood. One could suppress the coupling of radial and azimuthal structure, so that the radial dependence is a normal shear eigenfunction $\exp - (\frac{r-r_o}{\sigma})^2$, where r_o is the position of the mode rational surface, $\sigma = \rho_i (L_s/L_n)^{1/2}$ and L_s is the shear length. Even accepting all this, what does one take for ∇_r? It varies from zero on the mode rational surface to infinity far away. With so many fudge factors available, it is not at all surprising that theory can 'explain' experiments.

We now discuss the strong turbulence theories.[3-5] The basic assumption here is that when wave amplitudes become large, and the spectrum is highly turbulent the ion orbits are grossly perturbed. Instead of linear orbits, the ions diffuse across the field with diffusion coefficient D. Putting this additional diffusion in the ion continuity equation, it is a simple matter to show that it gives rise to a damping term $k^2 D$. Thus, when $D \sim (\gamma/k^2)_{MAX}$, every wave in the spectrum will be stabilized and one has the anomalous ion diffusion coefficient.

This simple and appealing strong turbulence picture also has many problems. First of all, if one adds $-k^2 D$ to the growth rate of each wave; when the last wave has been stabilized, all other waves have negative growth rate. Therefore the spectrum would quickly collapse into a single wave and violate the initial assumption of a turbulent spectrum. I believe that this conceptual problem must be faced and resolved in a convincing way before the strong turbulence theory can be used legitimately.

Secondly, the hypothesis that ion orbits diffuse is assumed and not proved. One can in fact give a very simple counterexample, a nonlinear drift wave.

For a drift wave, the ion fluid is described by cross field E x B drifts

$$\frac{\delta}{\delta t} n_i - \underline{\nabla} \cdot n_i c \frac{\nabla \varphi \times \underline{i}_z}{B} = 0. \tag{1}$$

where $\underline{B} = B\underline{i}_z$. If the electrons are Maxwellian,

$$n_e(x) = n_o(x) \exp \frac{e\varphi}{T_e} , \tag{2}$$

as they are for a drift wave, and the plasma is quasi-neutral,

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$$n_e = n_i \equiv n, \tag{3}$$

one can easily manipulate Eqs. (1) - (3) to give

$$\frac{\partial}{\partial t}\left(\frac{e\varphi}{T_e}\right) - \frac{cT_e}{eBn_o(x)} \frac{\partial n_o(x)}{\partial x} \frac{\partial}{\partial y}\left(\frac{e\varphi}{T_e}\right) = 0. \tag{4}$$

Notice that Eq. (4) has two remarkable properties. First, it is linear in φ even though no linearization was assumed. Second, it involves only $\frac{\partial}{\partial y}$ even though the spectrum was assumed to be fully two dimensional in x and y. Notice that not only is there no diffusion term or dissipation term of any kind, there is not even a vortex production term (i.e. a term involving derivatives in both x and y). Also, it is worth pointing out that instead of Eq. (2), one could have allowed n_e to be any function of φ and still ended up with a one dimensional linear equation. Thus it appears that ions can diffuse at large wave amplitude only because of corrections to Eq. (2), which are usually small. The strong turbulence theory has to resolve this difficulty in a convincing way. Even if the ions do diffuse, it is the electron thermal conduction and not the ion diffusion which seems to be the most relevant transport effect for tokamaks. Thus the ion diffusion must be related to electron thermal conduction. Finally it is worth pointing out that just as one shows from the fluid equations that ion diffusion is a stabilizing effect, one can also show that electron thermal conduction is a destabilizing effect. Implications of this for the strong turbulence theory are still unclear.

Another qualitative concept, is that of free energy.[9-11] A drift instability is assumed to be driven by expansion free energy in the plasma. For instance the authors of reference 12 assume that the free energy is that energy released by a plasma in adiabatically expanding to uniform density over a distance of the shear induced radial extent of the unstable wave. As the plasma expands, free energy is converted to wave energy. An upper bound to the wave energy therefore occurs when all free energy is converted to wave energy.

If the free energy and all constraints are properly calculated, such an argument is valid for an isolated plasma which expands freely. However, it is difficult to see how the argument can be applied to a tokamak plasma which is not isolated, but is in steady state. It is continuously receiving energy from the external circuit and losing it to the external environment. Thus as free energy is converted to expansion energy, it is simply replaced by the external circuit. If free energy arguments are to make any sense for tokamak plasmas, these arguments must be formulated for plasmas coupled to their environment, not for isolated systems.

I would now like to discuss the more quantitative turbulence theories [6-8] starting with mode coupling. NRL has solved several mode coupling problems,[13-16] and several similar problems have been worked out for parametric instabilities at other laboratories.[17,18]

The basic idea is that each mode amplitude $C(k)$ obeys an equation

$$\frac{dC(k)}{dt} = (\gamma(k) + i\omega(k))C(k) + \sum_{k'} M(k,k')C(k')C(k-k') \tag{5}$$

where $\gamma(k)$ is the linear growth rate, $M(k,k')$ is the coupling coefficient and $\omega(k)$ is the frequency of the mode with wave number k. The system of equations (5) can then be solved numerically for a large number of modes.

For those k' which have

$$\omega(k) = \omega(k-k') + \omega(k') \tag{6}$$

the mode coupling terms drive the mode at k at its own resonant frequency. It is at such k' that the effect of mode coupling maximizes. If. Eq. (6) is satisfied only for a small part of the spectrum, one can show[19] that Eq. (5) simplifies and reduces to a series of equations involving $|C(k)|^2$, that is all phase information drops out. On the other hand, if Eq. (6) is satisfied, or nearly satisfied for all waves in the spectrum, no simplification can be made and Eq. (5) must be solved as is, retaining all phase information. For drift waves and trapped electron instabilities

$$\omega(k) = kV_D + \Delta\omega(k) \tag{7}$$

where $\Delta\omega \ll \omega$ for $k\rho_i \ll 1$. Thus Eq. (6) is nearly satisfied for all k. If $\Delta\omega(k)$ were zero, Eq. (6) would be exactly satisfied for all k.

There are two general subcategories to Eq. (5). On one hand, the mode coupling terms may not dissipate energy, but only transfer the energy around in k space. That is

$$\sum_{kk'} M(k,k')C^*(k)\,C(k')\,C(k-k') = 0. \tag{8}$$

On the other hand the mode coupling terms may dissipate energy, or

$$\sum_{kk'} M(k,k')\, C^*(k)C(k')C(k-k') \neq 0. \tag{9}$$

An example of the former type of mode coupling is the steepening of a sound wave. An example of the latter is nonlinear Landau damping. It is worth pointing out that if Eq. (9) is satisfied, there is no reason a priori to assume the nonlinear terms give rise to damping. They may well give rise to growth.

Only the former, Eq. (8), has been investigated so far in tokamak context. From Eq. (8) one can easily show that in steady state

$$\sum_{k'} \gamma(k)\, |C(k)|^2 = 0. \tag{10}$$

Therefore in order for a steady state to form, there must be damping as well as growing waves.

While a growth rate spectrum like that shown in Fig. 1a can in principle give rise to a wave spectrum satisfying Eq. (10), our own experience with numerical solutions of Eq. (5) shows one cannot expect stabilization in this case. Rather the growth rate spectrum must have the form shown in Fig. 1b. The presence of dispersion (a non zero $\Delta\omega(k)$ in Eq. (7)) makes the situation even worse. For the cases studied in Ref. 8, 15, we found that with dispersion present, the growth rate spectrum must look more like that plotted in Fig. 1c. Solving Eq. (5) numerically with and without dispersion can easily mean an order of magnitude change in anomalous transport coefficient.

Thus nondissipative mode coupling only appears to be a viable stabilization mechanism if one is not far from instability threshold. In our own work[8] on the dissipative trapped electron mode, we found nonlinear stabilization only if drift resonances[20] were neglected. If drift resonances are included, the growth rate spectrum does not look like that shown in Fig. 1c and one does not find stabilization by nondissipative mode coupling. At this point, the theory must be regarded as a failure. In the similar Princeton work[7] on the trapped ion instability, dispersion was neglected even though some of the modes involved in the coupling have frequency not much less than the ion bounce frequency. Thus, this theory, as it is now, is at best highly questionable. Even taking its results at face value, one finds viable stabilization only at quite low temperature. The transport coefficient scales as $T^{19/2}$ far from marginal stability, and gives reasonable values only below about a kilovolt.

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It is possible that by including the dissipative effects of mode coupling, Eq. (9), some of the difficulties of mode coupling may be resolved. This has not yet been done. However it seems clear that in future papers in this area, a reader has a right to know just where the dissipation is coming from. Do the nonlinearities simply form a bridge between linear growth and damping, or do the nonlinearities provide some or all of the necessary dissipation?

Another quantitative theory which has proven to be a failure is electrostatic trapping. Since electrostatic trapping is such a potent stabilization mechanism for beam plasma, and ion acoustic instabilities, it is natural to suspect that electrostatic trapping along a magnetic field line could also stabilize a trapped electron instability. For the case of a single unstable wave in a square magnetic potential, the nonlinear calculation is actually quite straightforward. We have investigated this problem[3] and have found no stabilization until $e\varphi/T_e$ approachs totally unrealistic values ($e\varphi/T \sim 100\,\epsilon$). The basic difficulty seems to be that trapped particles don't much care whether they are trapped magnetically or electrostatically. They still drive instability.

I would conclude that the whole area of anomalous transport through nonlinear theory is not in very good shape at all. While there may be good experimental bases for various scaling laws for electron thermal transport from tokamaks, there is no solidly based theory. The multiple regime transport coefficients in WASH 1295 just cannot be taken seriously.

With nonlinear theories in such bad shape, the best remedy is lively and frank discussion by as many interested parties as possible. One obstacle to this is that many nonlinear theories are bewilderingly complex with theorists at their wits end. I think it is fair to say that most nonlinear theorists do not understand most nonlinear theories. What then is an outsider, say an experimentalist or contract monitor, to do? My feeling is that there are certain bases which a nonlinear theory ought to touch which nearly any scientist can understand, and also, there are certain well posed problems in existing theories. I hope this letter sets out some of these bases and conceptual problems so that other scientists can understand more easily what nonlinear theorists are trying to do. Conversely, I hope it encourages nonlinear theorists to touch these bases and address these problems in their own publication

Finally, my own feeling is that if tokamaks are to play an important part in this country's energy supply, research in this area should become more broadly based. It is the authors hope that this note will help to broaden the base in the area of nonlinear theory.
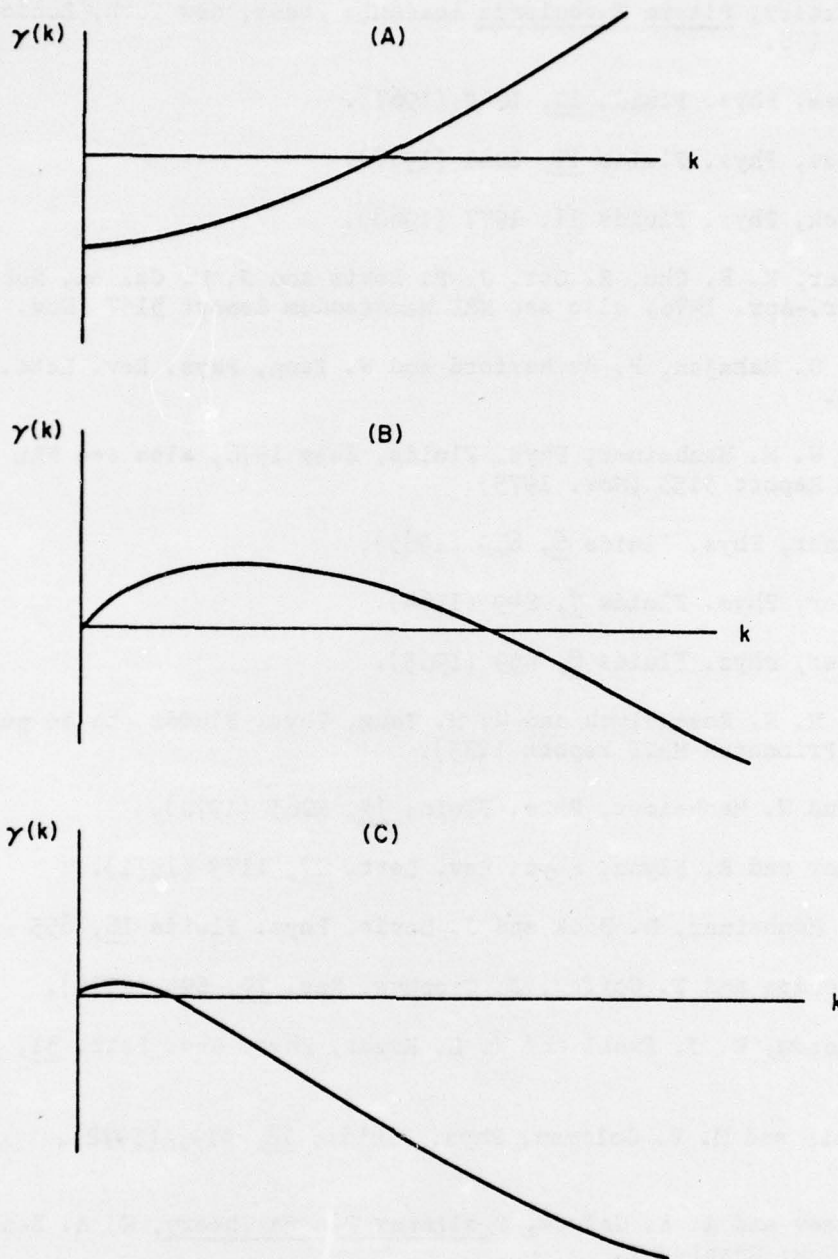
Fig. 1 — Schematic of the sort of growth rate spectrum for which mode coupling
is not (A and B) and is (C) a viable stabilization mechanism

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